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ABSTRACT

This paper focuses on the lack of productive knowledge-building in social education research by raising issues related to the place of scholarship in the field, the nature of its goals, and whether empirical-scientific research can be carried out to meet those goals. Attention is drawn to the limits of educational research based on untenable assumptions about human behavior so that more fruitful perspectives on the role of research may be developed and so that future research undertakings may be more likely to produce knowledge to inform and sensitize decision-makers. The paper also examines the following topics: (1) teachers' lack of access to research findings and the limited use of such findings; (2) the contradiction between two goals of research: the desire to help practitioners and the desire to contribute to the body of scientific knowledge; (3) the appropriateness of the empirical-scientific approach in building knowledge; and (4) how meta-analysis fits into the research picture. (KWL)

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SCHOLARSHIP IN SOCIAL EDUCATION: ISSUES FROM AN EMPIRICAL, META-ANALYSIS RESEARCH PERSPECTIVE

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As egocentric or intimidating as it may sound, we are part of the intellectual history of educational research and, in particular, of social education research, a field within which it is common to lament the lack of productive efforts at knowledge-building. Perhaps the most difficult thing that can be asked of anyone who is part of a Zeitgeist (as we are, in large part in social education, caught up in the dominant empirical-scientific spirit of educational research) is that they disregard their frames of reference, the specific commitments bred by their own past actions, and examine critically and thoughtfully, without defensiveness, the endeavors in which they are engaged. Our consideration of scholarship involving "scientific-empirical" research, including meta-analysis, in social education will be aided if we can adopt such a perspective.

I would like to pose some issues--some new, but most recurring, and unresolved, ones--in regard to our endeavors in that field. I could begin by spending considerable time in drawing fine definitional distinctions. However, I think there is sufficient general agreement in usage of the term "empirical research" or "scientific-empirical research" for the purposes of

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our discussion today. Stated briefly, it is the traditional type of social science research, with a positivist orientation, through which we have attempted to build knowledge, using largely quantitative assessments and methods assumed to be realistically (that is, validly) related to the "real world"--that is, to an orderly reality which is there to be discovered. (For a close approximation to this usage, see the definitions of "empirical-analytic research" by Popkewitz & Tabachnick, 1982, p. v, and Popkewitz, 1986, pp. 14-17.)

Scholarship By and For Whom?

My focus today will be on the frequently lamented lack of productive knowledge-building in our field. A mundane, but crucial, starting point is to note that of those college and university people who might be engaging in scholarship, and in particular empirical research in social education, very few are so involved. There is not an overwhelming body of scholarship, quantitatively speaking. Theory and Research in Social Education (TRSE) is not inundated with manuscripts; relatively few studies are published there in any one year, and very few studies in social education are published elsewhere.

Ours is basically a service profession, concerned with the education of young people and, for that reason, with the education of teachers. That orientation, while not necessarily contradictory to research interests, does take up resources of time and energy, and demands attention to a different set of concerns and questions. Viewed from that perspective, it is not surprising that research scholarship has not exactly proliferated in social education.

For Whom or What Purpose is Research Done?

Given the research that is done, for whom is it done or for what purpose? Some argue that they do research to affect practice. That is, their purpose is to provide knowledge that teachers, curriculum specialists and supervisors, and college methods professors can use in making curriculum and instructional decisions. For those people, it must be disconcerting that social studies teachers seem rarely to be aware of research findings, much less users of them (Shaver, Davis, & Helburn, 1980). But that should not be surprising. If asked ourselves how often we use research findings in making instructional decisions in our own courses, I would suspect that most of us would have to answer as did those in an informal survey of Stanford University faculty by Eisner (1983) and of Utah State University faculty by me (1982b): Despite their admonitions to public school people to base practice on research, education faculty rarely do so themselves.

There are, of course, a number of reasons why teachers pay so little attention to research, and find it of little help when they do. In one recent paper (Shaver, 1982b), I listed some fifteen possible reasons. Aside from teachers' lack of access to research findings, the limitations on use can be grouped in three clusters: (1) the research typically does not address issues or questions of interest to teachers; (2) research is typically not set in a context that is deemed valid by practitioners (for example, treatments tend to be of very short duration and carried out by people who are only temporarily in the schools); and, (3) research studies tend to produce inconsistent, even contradictory, results. All told, neither the relevance nor the implications for curriculum or instruction are clear.

On the other hand, some researchers would argue that their intent is to build knowledge--some say, theory*--about social education. For those who conduct research from a theory-building orientation, the major disconcerting reality must be that despite some impressive, suggestive studies, generally there is a lack of cumulative knowledge or empirically-based scientific theory in social education.

Perhaps part of the problem is that researchers often claim both goals--that is, their intent is to help practitioners and to build theory (see, e.g., Cornbleth, 1986, p. 7).** Thomas Kuhn (1970, pp. 19, 164) pointed out the contradiction between efforts to provide service through research and to build theory through research. Theory-building research must be, Kuhn argued, driven by the logic of the investigation and the availability of appropriate tools, not by the press of social needs. (One must be careful, as Kuhn was, not to overdraw this distinction between research driven by intellectual interests and that justified as meeting societal interests. Much basic research is motivated by concern for alleviating human suffering, for example, from cancer.) In social education, the paradox is that the research has met neither the service needs or the demands of scientific knowledge.

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*I will not define "theory" extensively here, as I think our common understanding of what is meant by "theory", that is, nomothetic, scientific theory, as commonly used from the empirical perspective will suffice: that is, theories ". . . 'ideally tell us the necessary and sufficient conditions for a particular result', allow the forecasting of outcomes 'with a reasonable margin of error', once parameters are specified, and include statements of the 'boundary conditions that limit [their] application' (Cronbach, 1975, p. 125; also see Larkins & McKinney, 1980, p. 14)" (Shaver, 1982a, pp. 2-3).

**For recent examples of discussions of the practice-theory dilemma for researchers in other fields, see Howard (1985), the responses to his article, and Applebee (1986).

It may be that there is insufficient scholarly research activity for either goal to be accomplished. Or, it may be that the empirical-scientific epistemological model is inappropriate for research in social education. That is, clearly, we lack the obvious progress from research which Kuhn (1970, p. 160-161) indicates is essential to science.* Also largely absent from the research in social education is at least one methodological characteristic of those fields that progress so obviously that they are labeled science, as Kuhn pointed out, with little regard for the niceties of definition--that is, a commitment to programs of research and to replication (see, e.g., Shaver & Norton, 1980). The question that follows is whether our scanty progress is due to lack of effort, faulty research strategy, or neither?

Is the Empirical-Scientific Approach Appropriate?

Is the empirical-scientific approach appropriate for building knowledge in social education? If that epistemology were properly implemented in social education research, would more useful answers for practitioners or theory be the outcome? Is the reality of interest to us susceptible to "scientific progress", as Kuhn (1979) used the term to refer to the natural sciences?

In that regard, I continue to be impressed by the arguments put forth by Gergen (1973) and Cronbach (1975) that findings in the social sciences do not hold still long enough to be accumulated as knowledge or built into theory.

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*Kuhn (1970) makes this point as a rather circular definition--that "the term 'science' is reserved for fields that do progress in obvious ways" (p. 160)--as an explanation for the lack of debate by those in the natural sciences as to whether their field is "really a science" as contrasted with the debates of that sort among "social scientists".

That is, while human behavior is orderly and lawful, those who would formulate the laws of human behavior must take into account the continuous cultural changes, including those due to research, that condition human frames of reference and the resulting behavior. Those who try to formulate such laws will always be faced with the yet to be revealed effects of history. (Also see Campbell, 1986.)

Cronbach (1975), going one step further, has pointed out that human behavior not only interacts with historical context, but with personological factors and ecological environments as well. The almost unlimited number of potential interactions to be ferreted out, Cronbach claims, makes the pinning down of the factors by which to explain behavior well nigh impossible (and perhaps accounts for many of the inconsistent, even contradictory, findings in educational research). (See Campbell, 1986, too.) The compelling and perplexing question raised by Cronbach's position is, what moderating variables ought to be taken into account in interpreting one's findings, but were not investigated, and perhaps could not be investigated because they are part of a context of application which has not yet occurred?

The purpose of research from the Gergen-Cronbach perspective must be not to build nomothetic theory, but to sensitize people to the range of factors that may influence behavior in specific situations and to the relative importance of these factors (Gergen, 1973, p. 317); or, as Cronbach (1975) put it, "to develop . . . concepts which will help people use their heads" (p. 126)--a position harmonious with those who, so far without much fruition, aim to improve educational practice through research. It also sounds strangely like the purpose of critical research as enumerated by Popkewitz (1986, p. 20).

Note again that Gergen and Cronbach do not reject the premise that human events are lawful, but only quarrel with the idea that the assumed regularities can be somehow formulated into nomothetic scientific theory. But perhaps even the assumption of the lawfulness and regularity of human behavior needs to be challenged in pondering whether empirical-scientific epistemology is likely to lead to cumulative knowledge and theory in social education. Charles Perrow (1981), a sociologist who spent a great deal of his career "discovering" and writing about social organization, has, in a provocative essay, attacked that "conventional wisdom" of the social sciences. Perrow contends that, contrary to the scientific-empirical assumption of lawfulness and regularity in human behavior, there is considerable nonrationality, natural disorder, and unpredictability to human life. This claim, of course, runs counter to the social science goal of building rational designs to explain human behavior and thus eliminate--through what Perrow refers to as "convenient fictions" (p. 3)--disorder and unpredictability.

Perrow argues that our drive to give "sensible accounts" of human phenomena--that is, to build theory--leads social scientists to disregard "happenstance, accidents, mysteries, illogicalities, and above all, fate" (p. 4) as unforseeable determiners of human lives and social events. That was not the case, he points out, with the ancients, who tried to "make sense out of things", but also accepted the limits of rationality in discerning patterns of personal and social life, to the extent that such patterns exist. As Perrow (1981) summed up: "Count no life happy, the chorus repeats in the Greek tragedies, until it is over; one can never know what the unpredictable gods have in store" (p. 4).

Taken together, the critiques by Gergen, Cronbach, and Perrow suggest a different representation of social education than that of discoverable, predictable regularities. Our failure to accumulate knowledge may not be due to our imperfect implementation of scientific methodology, but to the inadequacy of that epistemology to the reality which we are trying to comprehend. Nonquantitative methodologies, such as ethnography, will, I suspect, turn out not to be panaceas either, as their users strive to develop generalized propositions about social education. The data produced by absorption in the life of a few classrooms or through a critical perspective (see Stanley, 1986, p. 89) are likely to prove as inadequate a basis for generalization as traditional historians have, appropriately, perceived their data to be (Perrow, 1981).

Meta-analysis

Where does that new great hope, meta-analysis, fit into this picture? I suspect that the answer is fairly obvious. Let me note that I am in the midst of a federally-funded meta-analysis of some 273 reports, with over 700 effect sizes, of research to modify attitudes toward disabled persons. I had methodological as well as substantive reasons for wanting to do such a review of literature. I am interested in the effects of negative attitudes toward disabled persons and, therefore, in how those attitudes might be modified. But I also wanted the opportunity to conduct a meta-analysis so that I could come to a better understanding of the method, as I was an early critic (Shaver, 1979) of Glass' (1976) concept of meta-analysis. I argued then that it was an inadequate quantitative answer to the problems of insufficient knowledge accumulation created by overreliance on inferential statistics and

inattention to the basic scientific strategy of replication. I have not changed my mind.

All that I have said above about the accumulation of knowledge through empirical-scientific methodology applies to meta-analysis. I am becoming more convinced through my own work that we stand to gain little from quantitative syntheses of primary research studies that are poorly designed and unrelated to one another. Certainly, attempts at quantitative synthesis are not akin to replication in the scientific sense, as has been implied by some authors (Jackson, 1980, p. 445; Bangert-Downs, 1986, p. 398). Attempting to infer explanatory patterns retrospectively is an entirely different logic from that of the prospective process of replication to determine whether findings will be reproduced and within what limits (Platt, 1964).

A dogma among the rapidly emerging set of meta-analysis methodology specialists (e.g., Glass, 1976; Glass, McGaw, & Smith, 1981; Jackson, 1980; Rosenthal, 1984; Hedges & Olkin, 1985; Hunter, Schmidt, & Jackson, 1982; Wolf, 1986; Slavin, 1986) is that the steps in a quantitative review of literature are parallel to those in conducting a primary investigation. In that sense, those who do meta-analyses face--or should face--many of the same problems as those carrying out primary research studies. For example, the sampling problem which has plagued primary researchers (e.g., Shaver & Norton, 1980) is handled to some extent by the tactic, which we have adopted in doing our integrative review, of obtaining as complete a set as possible of published and unpublished research on the topic--that is, by attending to an accessible population rather than to a sample.

The most troublesome aspect of a meta-analysis to me has its direct parallel in primary research, that is, the adequacy of assessment devices.

There is, in my experience, little difficulty in attaining reliability in the coding of study characteristics, but serious questions of validity are raised by the difficulties in anticipating and capturing the nuances of various primary studies conducted under differing conditions with varying samples and with the research reported with widely divergent degrees of detail and insightfulness into the elements of design and execution that might have moderated effects. The validation of meta-analysis coding instruments has hardly been treated in the literature, certainly not adequately. I urge you to scrutinize carefully the Instrumentation section of any meta-analysis report before deciding whether to rely on the findings and conclusions.

Also bothersome is a strong push to once again rely on inferential statistics in analyzing data, ignoring both the inadequacies of number-dependent indicators of significance and the lack of relevance of statistical inference when analyzing data from populations rather than samples (although there is the sticky connundrum of whether to consider the primary studies being reviewed or the populations and settings in which the findings might be applied as the appropriate target in drawing conclusions--see, e.g., Jackson, 1980, p. 453). Once again, as Wilson and Rachman (1983) have pointed out, there is the "danger that [the use of] sophisticated statistical techniques [will] serve [to] obscure damaging flaws in the evidence" (p. 55). And there is the ever present danger of fallacious reasoning based on misplaced precision as study characteristics are coded as numbers and effect sizes are computed to two or more decimal places.

Participation in the coding of our 273 studies has given me a feeling for the myriad aspects of design and context that can affect results. Hunter, Schmidt, and Jackson (1982, pp. 32, 139) have pointed out that, in their experience, the greatest percentage of variability in outcomes among

primary studies can be accounted for by contextual factors and research artifacts, including sampling error, leaving little to be explained in terms of the treatment variable. I suspect our analysis will support that conclusion.

If a meta-analysist wants straightforward answers, a simple coding system should be used so that the many possible interactions between treatments and contextual factors and research artifacts do not have to be taken into account. The analogue, of course, would be the use of one-way analysis of variance, rather than complex designs, so that one does not have to confront the difficulties in interpretation imposed by statistically significant interaction effects, much less consider all of the potential interactions which have not been investigated but which likely confound the interpretation of results.

I believe it was Chaim Potok who pointed out that for most people the need to know is much greater than the need to know what is true. I suggest that his insight explains much of the interpretation of research in education, perhaps applying especially to the answers being sought via meta-analysis.

Summing Up

The tone of this presentation is not meant to be negative, but hopefully provocative in a dialectic sense. I have tried to raise some issues related to the web of circumstances and epistemological difficulties that appear to bind and confound our efforts at research in social education from the empirical-scientific perspective. These issues include: What is the place of scholarship in social education? What are the goals of that scholarship (assuming that they are more than having publications for the sake of tenure

and promotion)? And, in particular, can research, especially empirical-scientific research, but ethnography and critical analysis as well, be carried out in such a way as to meet those goals?

Calling attention to the unpredictability of human behavior, even to the role of fate, is not intended to be an argument against our efforts to be rational. It is meant rather to call attention to the bounds of rationality, to use Simon's (1983) terminology, and to the limits of educational research that is based on untenable assumptions about human behavior, in order that we might develop more fruitful perspectives on the role of research. Our past research endeavors in social education have not been particularly productive of either scientific theory or knowledge to inform or sensitize decision-makers. The first does not seem likely. How to better produce the latter is an important agenda item for social education researchers. (Parts of this section are taken from Shaver, 1982a, b.)